

undiminished. It was, however, as its author says, mainly intended for the students of philology; and a simpler and more practical manual, therefore, was called for by the teachers of the deaf and dumb, the missionaries in foreign countries, the elocutionists and the trainers of common school teachers, who have all made more or less extensive use of it. Their demand has, accordingly, been supplied by a clear and compact manual, in which the character, varieties, and relations of phonetic utterances are explained by the help of the symbols of visible speech. The book begins with an explanation of the symbols themselves, and then goes on to analyse and distinguish the consonants and vowels, describing their physiological formation with a clearness of language and an appeal to the eye, which ought to enable the most backward of learners to reproduce the greater part of them after a little practice. The value of this section of the manual, to those who wish to acquire the pronunciation of a foreign dialect, need not be pointed out. It is only a pity that there are no means for enabling the ear of the ordinary speaker to detect the differences of sound, which, when once written down in "visible speech," he ought to find slight difficulty in reproducing. No method, however, has yet been discovered of training the ear, as Mr. Bell has succeeded in training the physiological organs of speech. What this success is, may be judged of from the last table given in the volume, in which such elementary sounds as sobs, coughs, yawns, sneers, or even a smoker's puff, are expressed by symbols that can be at once understood, and translated into audible sounds. The fourth section of the volume contains specimens of English, Lowland Scotch, French, and German, with their ordinary pronunciation exactly noted in Mr. Bell's symbolic alphabet, while the last section consists of a supplementary review of the essentials of articulation, with a couple of concluding pages on "the application of visible speech to the teaching of articulation to the deaf."

LETTERS TO THE EDITOR

- [The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]
- [The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Conservation of Solar Energy

WHILE Dr. Siemens' novel and ingenious theory described in his paper before the Royal Society, and published in NATURE, will doubtless be adequately criticised in its more physical aspects by those who are better acquainted than myself with "the intricacies of solar physics," I may perhaps be allowed to point out one or two conclusions which appear somewhat opposed to the laws of mechanics. The author, for example, lays great stress upon the "high rotative velocity of the sun," which at the solar equator, according to his figures, is four and a half times that at the terrestrial equator. To this "high rotative velocity" Dr. Siemens attributes the supposed expulsion from the solar equator of the products of combustion of the gases drawn in by the assumed fan-action at the solar poles.

Mairan apparently thought the equatorial rise of the solar atmosphere due to the centrifugal force engendered by this velocity sufficient to account for the appearance of the zodiacal light, and according to Dr. Siemens his supposition may possibly be correct, if we suppose that space, instead of being an aether-vacuum, is filled with highly-attenuated gases. It seems, however, that La Place, acting on the usual supposition of an empty stellar space, calculated that the solar atmosphere could not extend more than 9-20ths of the distance of Mercury, or about 16,000,000 miles, at which distance it would exist in such a highly rarefied condition as almost to merit the designation of vacuum. That this must be so, is evident when we remember that the high superficial velocity at the solar equator, though relatively larger than

that at the terrestrial equator in the proportion given by Dr. Siemens, so far from being able to exert a powerful centrifugal force, is in this respect far less effective than the smaller tangential velocity at the terrestrial equator. This is chiefly due to the counteracting influence of solar gravity, which, as is well known, is more than twenty-seven times terrestrial gravity as represented by g . It is also partly due to the large value of the solar radius, since this also enters into the denominator of the expression for centrifugal force in terms of the tangential velocity, viz. $\frac{v^2}{r}$. It is at least remarkable that Dr.

Siemens has made no allusion to either of these factors, which so intimately affect the centrifugal efficiency of the centrifugal force—the motive-power on which the entire action depends—and has made it appear from his language that this is a mere simple function of the tangential velocity at the solar equator.

As it is, owing to these united circumstances, but mainly to the former, the ratio of the centrifugal force acting on a particle to its weight is, even at the solar equator, almost infinitesimal.

To accentuate this astronomical platitude it is only necessary to quote figures which may be found in every popular work on the sun, such as the fact that while the centrifugal force at the terrestrial equator deprives a body of 1-289th of its weight at the poles, the amount it would similarly lose at the solar equator would be only 1-18,000th. Or again, to put it in another light, in order that solar gravity and centrifugal force may equilibrate, and a particle at the solar equator be without weight, the sun would have to turn upon its axis 133 times as fast as at present, while in order that the same conditions should prevail on the earth, its rotational velocity would only need to be increased seventeen times.

Except, therefore, where they would be momentarily affected by the local expulsive forces engendered by solar combustion, the different layers of the solar atmosphere would arrange themselves in the order of their relative densities, and remain quietly attached to the surface of the sun, under an attraction fully twenty-seven times greater than that which our earth exerts on its aerial envelope. That, under such circumstances, the centrifugal force of the sun could cause it to project into space the products of combustion, seems most improbable.

Moreover, suppose, for the sake of argument, that this action really does take place, can it be literally maintained, according to Dr. Siemens' concluding sentence, that this action is "capable of perpetuating solar radiation to the remotest future"? The laws of energy tell us that work cannot be done without expenditure of energy, and since the "primum mobile" in this case is solar rotation, and the gases entering at the solar poles must gradually acquire rotational momentum at the sun's expense, they must, in time, reduce it to naught, when the supposed regenerative action would cease, and so the sun burn out. In any case, therefore, the word "remotest" can only be understood to have a limited signification.

E. DOUGLAS ARCHIBALD

[To save time we submitted Mr. Archibald's letter to Dr. Siemens, who sends the following reply.—ED.]

This letter shows that Mr. Archibald has missed the principal point of my argument concerning solar fan-action. I showed pretty clearly I thought that solar gravitation would affect the inflowing and the outflowing currents equally, and that centrifugal action must determine motion in the equatorial direction in a space filled with matter. But to put the problem into a mathematical garb let us consider the condition of two equal masses m_p and m_a , both at the radius R from the solar centre, the one opposite either pole, and the other opposite the equatorial region. The moment of gravitation of both these masses will be represented respectively by $\frac{gm_p}{R^2}$ and $\frac{gm_a}{R^2}$, and supposing both masses to be gaseous, and of the same chemical composition and temperature, they will represent equal volumes, say one cube foot.

These conditions being granted, we may put—

$$\frac{gm_p}{R^2} = \frac{gm_a}{R^2},$$

but the mass m_a is subject to another force, that produced by tangential motion, which shall be represented by v , and the centrifugal force resulting from this motion by ϕv ; the moment of gravitation towards the sun will then be reduced to $\frac{gm_a}{R^2} - m_a \phi v$, and the latter factor being a positive quantity we have—

$$\frac{g m_p}{R^2} > \frac{g m_a}{R^2} - m_a \phi v.$$

This inequality of attractive moments must determine motion toward the sun in favour of $\frac{g m_p}{R^2}$, and this condition holding good

for any value of g and R , it follows that the polar inflow and equatorial outflow must take place, provided only that space is not empty, as supposed by La Place, but filled with either an elastic or non-elastic fluid.

To put it in another way, Mr. Archibald imagines that in order to determine an outflow from the sun it is necessary for the centrifugal moment $m_a \phi v$ to exceed the moment of gravitation $\frac{g m_a}{R^2}$, whereas according to my view, the value of the former

determines only the rate of outflow, but is immaterial as regards the principle of action. The projection of dust is entirely dependent upon the outflowing current. I leave it for Mr. Archibald to determine for himself the velocity of current necessary to move a particle of dust of given size and weight away from the sun in opposition to its force of gravity, which I am well aware is twenty-seven times that of the earth on its surface.

The gaseous current is of course produced at the expense of solar rotation, but this expenditure of energy is relatively much smaller than that lost to our earth through tidal action, and may be neglected for our present purposes. It is moreover counter-balanced by solar shrinkage as explained in my paper.

C. WM. SIEMENS

Review of "Aristotle on the Parts of Animals"— A Correction

SINCE the publication of my review of "Aristotle on the Parts of Animals," a correspondent has called my attention to an article by Prof. Huxley, "On Certain Errors respecting the Structure of the Heart attributed to Aristotle" (see NATURE, November 6, 1879), in which the Professor corrects the common error, attributed to Aristotle, of describing the heart of the higher animals as possessing three cavities only. In ignorance of this fact I assigned the merit of originally detecting the error, so long attributed to Aristotle, to Dr. Ogle, who tenders, I have no doubt quite independently, the same defence of the matter. I now write to give the priority of the detection of the error to Prof. Huxley, and to thank my correspondent for having afforded me an opportunity of studying a most original and instructive essay.

BENJAMIN WARD RICHARDSON

25, Manchester Square, March 27

Deep-Sea Exploration in the Mediterranean

I SHALL be obliged if you will kindly announce in NATURE that, taking into consideration the vote expressed at one of the plenary meetings by the Third International Geographical Congress at Venice, the Italian Government has decided that the deep-sea exploration in the Mediterranean be continued during the forthcoming summer; and towards the end of July or beginning of August next I am to embark on board the surveying steamer *Washington*, Royal Italian Navy. About one month will be devoted to deep-sea exploration under the able direction of Capt. G. B. Magnaghi, R.N.

The study of the animals collected during last year's cruise will be completed with that of those we hope to collect next summer. Since presenting my Preliminary Report to the Geographical Congress, I have looked more carefully into the fishes collected last year; amongst them are two specimens of the rare *Malacocephalus levis*, Lowe, dredged in 508 metres off the south coast of Sardinia, and in 823 metres off Mauritius (Egadi, Sicily); and two specimens of the still rarer *Coryphenoides serratus*, Lowe, new to the Mediterranean fauna, dredged from depths of 2805 and 2904 metres off the west coast of Sardinia.

Dr. J. Gwyn Jeffreys was here a short time ago, and has examined the mollusca, on which he will report.

HENRY HILLYER GIGLIOLI

R. Istituto di Studi Superiori in Firenze, March 23

The Basque Whale in the Mediterranean

I WAS very much interested in Mr. Clement R. Markham's most important communication on the "Whale Fishery" in the Basque provinces of Spain, produced in NATURE (vol. xxv. p. 365). Mr. Markham has carefully collected important materials

for the history of a whale (*Balena biscayensis*), which, if not quite extinct, appears to have become so, to all intents and purposes, in a region where it was once so common as to have given rise to an important industry, and to have had a powerful influence on the habits of the Basque people along the northern coast of Spain. Mr. Markham gives solitary instances of the appearance of the whale off the Basque coasts, up to a very recent period, and says that the last instance of its occurrence which came to his knowledge, was on February 11, 1878, when a whale was sighted off Guetaria, and successfully harpooned. This bit of news must have interested all cetologists, and I hope that it may interest Mr. Markham and the readers of NATURE to know that a fine, nearly adult female of *Balena biscayensis* was captured just one year before, in the Mediterranean, viz. on February 9, 1877, at Taranto. It was ably and fully described by Prof. F. Gasco (*Mem. R. Acad. Scienze di Napoli*, vii, 1878); the entire skeleton is in the Museum of Comparative Anatomy in the University, Naples, in the Central Collection of the Italian Vertebrata at Florence. I preserve a portion of the skin of the snout, with short hairs, and a model of the entire creature, reduced to one-twelfth, carefully executed from drawings and measurements taken from the whale immediately after death. I know of no other recorded instance of the capture of a true whale in the Mediterranean.

HENRY HILLYER GIGLIOLI

R. Istituto di Studi Superiori in Firenze

Wind Measurements

AFTER reading the interesting article in NATURE, vol. xxv. p. 486, on wind measurements, the reader cannot but revert to the very unsatisfactory state of anemometry as it now exists. This is only too apparent from the reports which appear in the papers after a gale, and in which are generally detailed the estimated pressures and velocities of the wind as recorded by the anemometers at the principal meteorological stations. Thus during the gale of the 13th-14th of October last we were told that a pressure of 53 lbs. per square foot was recorded at Greenwich, and at the Bidston Observatory, Birkenhead, the pressure reached the alarming figure of 79 lbs. Now it may be readily shown without much calculation, that such pressures as these few buildings could withstand that were not of more than ordinary stability, not to mention the destruction of tall factory chimneys, which, when of the usual dimensions, will not stand a pressure of 30 lbs. per square foot. Yet no such destruction took place. I think, then, we must confess with T. Hawksley, F.R.S. (vide paper read before Section of British Association meeting, York, 1881, on Pressure of Wind on a Fixed Plane Surface), that our present anemometrical instruments are little better than philosophical toys.

C. H. ROMANES

Worthing, March 27

IN the account of D'Ons en Bray's anemometer, which I printed to accompany a drawing of that instrument at the Meteorological Society's Exhibition the other day, I stated that it was probably the earliest registering anemometer. I now find that I am mistaken, but as I erred in company with the President of the Society, I feel that I may well be excused. Until a still earlier instrument turns up, the idea of a registering anemometer must be ascribed to Sir Christopher Wren. In 1663 (see Birch's "History," i. 341, plate iii.), he laid before the Royal Society an account of his "weather clock," which is in fact a recording anemometer, but for direction *only*, together with an instrument for "showing degrees of weather," probably a self-recording barometer, but the description is not clear. The spindle which drives the hour-hand of the clock carries a pinion which moves a rack, long enough to pass out clear of the case on each side. At the end of the rack there is a pencil, which bears upon a disc keyed on to the spindle of the direction-vane. The disc carries a printed diagram, a series of radial lines indicating direction, the time being shown by a number of concentric circles. The irregular line drawn by the pencil records the direction of the wind. A fresh paper is placed on the instrument every twelve hours.

Whilst upon this subject, perhaps I may be allowed to call attention to a paper by Richard Lovell Edgeworth, on wind pressure, in the *Phil. Trans.* for 1783, p. 136. It contains the results of a series of experiments undertaken to determine the variations in the pressure of the wind upon surfaces of equal area, but of different forms. This is, no doubt, the paper referred to by Robinson, as the source whence he derived the